

To J. A. Lawrence
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JOSEPH PRIESTLEY AND HIS PLACE IN THE HISTORY OF SCIENCE

FROM the death of Newton in 1727 to the delivery by Dalton of his first lectures on the Atomic Theory, in Manchester, and in this building, at the end of 1803 and the beginning of 1804, this country made no great contribution to the theories of physics or chemistry. But it made contributions of fundamental importance to our knowledge of the facts essential for any real advance, and perhaps most conspicuous among our scientific pioneers of the eighteenth century stands Joseph Priestley, known chiefly as the "discoverer of oxygen," a strange and puzzling figure. Cuvier tells us that he was a father of modern chemistry who would never acknowledge his own daughter; Huxley, who, like others, so generously praises his high moral qualities and his discoveries, says that Priestley had no deeper comprehension of his own work. Roscoe and Schorlemmer, in their text-book, say

that "Priestley's was a mind of rare quickness and perceptive powers, which led him to the rapid discovery of numerous new chemical substances, but it was not of a philosophic or deliberative cast," and that "he was unable to grasp" the Lavoisierian theory of the composition of water. Sir Edward Thorpe tells us that "he was in no real sense a speculative philosopher" and was "entirely lacking in the higher qualities of the imagination." * It is of Priestley that Sir Oliver Lodge writes that "in theory he had no instinct for guessing right, such as the great men of science have had—an intuitive feeling for the right end of any stick: he may almost be said to have had a predilection for the wrong end; and that fact puts him out of the first flight of scientific men. He was a skilful and painstaking experimentalist, and possessed scientific ability of a high kind, but never does he approach the level of Faraday or of James Watt." †

I have called Priestley a puzzling figure. It is in the hope of solving, at any rate partially, the puzzle of his scientific thought and career that I am speaking to you to-night.

Priestley was many other things besides being a man of science; he was a teacher, a theologian, a politician, a fearless defender of liberal thought and of toleration for opinion of every kind, from the most orthodox to the most heterodox, a man whom Frederic Harrison describes as, though not the greatest mind, the hero of our eighteenth century.‡

* *Joseph Priestley*, by T. E. Thorpe (1906), p. 168. This is a useful little biography, though not always accurate in detail.

† *Nine Famous Birmingham Men*, edited by J. H. Muirhead (1909), p. 23.

‡ Quoted by Thorpe, *loc. cit.*

PRIESTLEY AND HIS PLACE IN SCIENCE

Priestley was born near Leeds in 1733, the son of a cloth-dresser who belonged to the Congregationalist body. At the age of eleven he bottled up spiders to see how long they could live without fresh air. He went to Batley Grammar School and later to a dissenting academy at Daventry, where he began to drift towards the Unitarian doctrines which he ultimately professed. At twenty-two he became a minister, at the princely salary of £30 a year, at Needham Market in Suffolk. His theology was devoid of all pomposity, and when he was a candidate for a post at Sheffield he was rejected as being "too light and airy." But he was accepted as a minister at Nantwich; and here in 1758 he made his first experiments in science, to please himself and the thirty or forty pupils at his school, to whom he never gave a holiday. He made them with the help of a few scientific books, a small air-pump, and an electric machine, bought with his pupils' fees.* In 1761 he became tutor in languages and belles-lettres at the well-known Warrington Academy; and during annual visits to London met Benjamin Franklin, William Canton, Sir William Watson, and other scientific men interested in electricity. In 1767 he became minister of Mill Hill Chapel at Leeds. In 1773, after he had won fame as a scientific man, he was engaged as librarian by Lord Shelburne (afterwards Marquis of Lansdowne) who, he says, treated him as a friend, and gave him full time for his own scientific work, which he carried on at Calne in Wiltshire and in Lord Shelburne's house in London. A

* Priestley's *Memoirs* (1806), pp. 40-42, and *passim*. His "scholars were generally the operators and sometimes the lecturers too."

temporary coldness arose between the two men in 1780, and in that year Priestley went, with a pension from Shelburne, to Birmingham, then the home or resort of many scientific men: Boulton and Watt, the engineers; Withering, physician, botanist, and mineralogist; Samuel Galton; Josiah Wedgwood, the potter; Keir, the chemist; and Erasmus Darwin—most of them members of the famous Lunar Society.*

Priestley stayed in Birmingham till 1791, when his house and laboratory were burnt down by the mob, excited by persons who had denounced his sympathies with the French Revolution.

Priestley then came to London, where he delivered a simple course on chemistry and taught history at Hackney New College. Here his life went on for a time "even more happily" than ever before. He was not unduly moved, it seemed, by the denunciations of Burke, by threats of being slowly burnt alive, or by being actually burnt in effigy.† In English politics he was a royalist and a constitutionalist, and the attacks were undeserved.‡ In 1792 he was made a citizen of France, though he declined election as a member of the French Convention. He found that his colleagues on the Royal Society shunned him because of his religious and political opinions,§ and he was already deeply hurt because they had rejected on political grounds his friend Cooper, whom he had proposed. In 1794, under the general political pressure, he left England to join his sons in the United States, and, continuing his scientific and literary work to the last, died there ten years later, in February, 1804.

* See *Dr. Darwin*, by H. Pearson (Dent, 1930).

† *Memoirs*, p. 154.

‡ *Memoirs*, p. 135.

§ *Memoirs*, p. 120.

During his career Priestley published some fifty works on theology, thirteen on education and history, and about eighteen on political, social, and metaphysical subjects. In addition he published twelve books and some fifty papers on scientific subjects, a colossal amount of work for any one man.*

While theology was always his chief interest, his preference for science over literature was so great that it bursts out irrelevantly in the most unexpected places—in his *English Grammar* published in 1761, and in his *Chart of Biography* of 1765. “Few,” he wrote,† “are qualified to make new discoveries of importance ; . . . but when discoveries have been made, and the principles of science have been ascertained, persons of inferior abilities . . . are sufficient to digest those principles into a convenient method.” The diffident Priestley is obviously thinking of himself as one of these “inferior persons.”

It was Benjamin Franklin, the amateur of genius, who encouraged Priestley to undertake his first scientific work, a *History of Electricity*, which was printed in 1767, within twelve months of its inception. The History, he says, drew him into a large field of original experiments, and on the strength of these he was elected F.R.S. on June 12, 1766. He describes his own reflexions and experiments in a simple, exact, and artless style, borrowed, as he admits, from another electrician, Stephen Gray, a style which contrasts with the excessive fluency of much of his purely literary work. It is in this History and in

* See articles in the Dictionary of National Biography by Rev. A. Gordon and the present writer.

† In the *Description of a Chart of Biography*, 2nd edition, 1765.

his electrical work, neglected by most of his biographers, that we see the scientific Priestley in some ways at his best and that we must look for the key to his mind.

The comparison of Watson and Franklin's single-fluid theory of electricity* with the two-fluid theory of his predecessors, leads Priestley to a remarkable confession of faith on philosophical, by which he means scientific, theories in general. The object of science, he tells us, is "to comprehend things clearly, and to comprise as much knowledge as possible in the smallest compass."† Though Priestley obviously owes much to Bacon, and perhaps something to Locke, the formula is, I think, a new one, at any rate in the mouth of a scientific man. It comes singularly close to the famous formulæ of Kirchhoff and of Mach, who defines science in effect as a shorthand description of Nature, leaving no room for cause or effect.‡ Priestley seems to depart from that view in his own discussion of the question of cause and effect, on which all scientific metaphysicians, up to the most recent and one of the most brilliant, Meyerson, argue "about and about."§ But I am not sure whether, in spite of his use of the terms, cause and effect, Priestley's view is really distinguishable from that of Mach. He has a clear vision of the value of hypothesis in scientific investigation. Every experiment, he tells us, in which there is any design, is made to

* Priestley holds the balance even between the two theories, though he prefers the single-fluid theory.

† *History of Electricity*, 1st edition, p. 442.

‡ See E. Mach, *The Science of Mechanics* (Eng. trans. 1893, p. 483). "There is no cause nor effect in nature . . . nature simply is." See also Karl Pearson's *Grammar of Science*.

§ See E. Meyerson's *Identity and Reality* (3rd edit., translated from the French, 1930).

ascertain some hypothesis, for an hypothesis is nothing more than a preconceived idea of an event. An hypothesis absolutely verified ceases to be termed such, and is considered as a fact. Hypotheses lead persons to try a variety of experiments in order to ascertain them, and in these experiments new facts generally arise which serve to correct the hypothesis which gave rise to them. By this method of successive approximations we may hope to discover all the facts and to form a perfect theory of them.* Thus science is to be reduced to a statement of all the facts in the smallest compass, and the hypotheses finally disappear. Yet Priestley warns us that “a philosopher who has been long attached to a favourite hypothesis, and especially if he have distinguished himself by his ingenuity in discovering or pursuing it, will not sometimes be convinced of its falsity by the plainest evidence of fact,” and thus “both himself and all his followers are put upon false pursuits, and seem determined to warp the whole course of nature, to suit their manner of conceiving of its operations.”

The object of the whole process of experimenting and reasoning Priestley, following Lord Bacon, regards as power and happiness. (It was to Priestley that Bentham is said to have attributed, though perhaps wrongly, the original of his utilitarian formula—“The greatest happiness of the greatest number.”)

In the exposition which I have summarised we see—

(1) That though Priestley the theologian re-

* *History of Electricity*, p. 445. He says: “by this perfect theory, I mean a system of propositions accurately defining all the circumstances of every appearance, the separate effect of each circumstance, and the manner of its operation.”

gards the whole course of events as due to a Supreme Cause, yet the exposition is too short to be sure whether, like Descartes and Boyle and Newton, he conceives at this period the external world as a form of mechanism ; but if he does so, it would seem as if, unlike Boyle and Newton, he finds no place in it, after it has once been created, for a Deity regulating and amending it from time to time.* He is, as Mr. Alexander Gordon says, a determinist. He professes himself elsewhere a disciple of the physical point theory of Boscovich, but he does not seem to have been influenced by it in his physical work.

(2) Hypotheses, though useful, seem destined to disappear ultimately from Priestley's scheme of the universe. There is no room in it for cause and effect. In this he was influenced, no doubt, by Franklin, whose indifference to his own theories and to those of others he quotes with approval. It is not, says Franklin, "of much importance to us to know the manner in which nature executes her laws. It is enough if we know the laws themselves. It is of real use to know that china, left in the air unsupported, will fall and break ; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it." †

Franklin's attitude is very far from being that of the author of the theory of gravitation. The famous passage in which Newton says, *hypotheses non fingo*, "I frame no hypotheses," is too often quoted without its almost immediate context in the *Principia*, that paragraph in which Newton

* See A. J. Snow's interesting *Matter and Gravity in Newton's Physical Philosophy* (1926), pp. 63, 205, and *passim*.

† *History of Electricity*, p. 467.

speaks as if with equal conviction of the ether, “a certain most subtle spirit which pervades and lies hid in all gross bodies”; a spirit to whose action is to be attributed heat, light, and electricity, and the excitation and transmission of the nervous impulse. Newton admits that there are insufficient experiments to demonstrate the laws by which this “electric and elastic” spirit operates.* And then, besides the ether, we have the famous “solid, massy, hard, impenetrable, moveable particles” created by God in the beginning,† the atoms of which bodies are composed, and on whose properties they depend: the atoms inherited by Newton through a long line of philosophers from the Greek atomists.

It has been pointed out in a recent book by Madame Hélène Metzger ‡ that eighteenth-century chemistry owes more to Newton than has generally been recognised. Priestley, at any rate, refers to him again and again. I think it would be roughly true to say that in the electrical investigations we see mainly the influence of the first Newton, the Newton of economical thought, of the *hypotheses non fingo*, and of the almost positivist Franklin. When we come to the chemical work we find the influence of the second Newton, the Newton of those passages in the *Principia* to which I have referred, and to what Priestley himself calls the “bold, eccentric thoughts” of the *Queries* and the *Optics*.§

* *The Mathematical Principles of Natural Philosophy*, translated by A. Motte (1729), vol. ii, pp. 392–3.

† Newton’s *Opticks*, 3rd edition (1721), *Queries*, p. 375.

‡ *Newton, Stahl, Boerhaave et la doctrine chimique*, by Hélène Metzger. (F. Alcan, 1930.)

§ The reference is no doubt especially to *Queries* 30 and 31 following the *Opticks*. See Priestley, *Experiments and Observations on . . . Air*, vol. i (1774), p. 259.

Let us now consider Priestley's main work in electricity.

That work was done long before the days of the voltaic cell and the constant current. Priestley's chief instruments were electric machines, made for him by Nairne, and Leyden jars. One of the chief properties that interests him is that of conductivity, a property first discovered by Gray, though the term "conductor" was first used by Desaguliers. Priestley shows that besides the metals, charcoal, black-lead, and red-hot glass are conductors, and that flame makes the surrounding air a conductor.* He hazards the suggestion that there is no absolute distinction between conductors and non-conductors, but that there is a scale of conductivity in which all bodies may be placed. I quote his words: "Independent of moisture, there is probably a gradation of substances, from the most perfect conductors to the most perfect non-conductors of electricity." †

In 1769 Priestley found that if electricity were made to pass through a conductor in the form of a long, open loop it was always possible to narrow the gap to a distance at which a spark would pass across, a part of the electricity choosing a short passage through the air rather than a long one through the conductor; and he says: "In this method the different degrees of conducting power in different metals may be tried; using metallic circuits of the same length and thickness, and observing the difference of the passage through air in each." ‡ We know now that with the

* *History of Electricity*, p. 612 *et seq.*

† *History of Electricity*, 1st edition, p. 435, and 2nd edition, p. 411. He inserts the word "probably" in the second edition.

‡ *Phil. Trans.* for 1769, p. 69.

variable currents which Priestley was using, he had to do with what is now called impedance and not only with resistance. It is interesting to note that Priestley tried to find whether the conductivity of a circuit depended on its configuration, but he failed to discover the effect for which he sought.*

Priestley is, in all his work, trying not only to observe qualitatively, but to measure. In some of his experiments he measures his discharge by the length of iron wire which it will fuse when inserted into a given circuit of brass wire.

We find the same interest in quantitative work in an experiment made at the suggestion of Franklin, who wrote in 1766 telling him that two pith balls introduced into an electrified metal cup showed no sign of electrification. Priestley confirms the experiment, performs it in a more convincing way, and asks the following question :

“ May we not infer from this experiment that the attraction of electricity is subject to the same laws with that of gravitation, and is therefore according to the squares of the distances (*he obviously means inversely as the squares of the distances*) ; since it is easily demonstrated that, were the earth in the form of a shell, a body in the inside of it would not be attracted to one side more than another.” †

It was probably this first announcement of one of the fundamental laws of electrostatics by Priestley that led his friend Cavendish to under-

* *Loc. cit.*, pp. 64, 65. He found that a zigzag wire and a straight one of the same length had the same effect in transmitting the discharge of a battery of Leyden jars.

† *History of Electricity*, 1st edition, p. 732.

take in 1772-3 that complex series of investigations on the law of the inverse square which was only published more than a century later under the editorship of Clerk Maxwell and Sir Joseph Larmor. As we know, the world owes the discovery of the fundamental law of the inverse square in electrostatics to Coulomb (1785). This work of Priestley's has found very little acknowledgment in histories of science.

Even less known than these quantitative experiments are a long series of experiments on what Priestley calls the lateral explosion.* He found that under certain conditions a rod connected with a Leyden jar will send a spark to an insulated conductor when the jar is discharged through an imperfect circuit, and that although the spark has struck the insulated conductor, that conductor is found to be electrified neither positively nor negatively. "In all other cases," he says, "the electric matter rushes in a single direction; whereas in this it goes and returns in the same path; and as far as can be distinguished in the same instant of time so that all the differences of the two electricities, which are so conspicuous *in vacuo*, must here be confounded."† To test his hypothesis he makes the explosion (as he calls it) take place *in vacuo*, with the poles about two inches apart, and finds a new appearance—a uniform "thin blue or purple light," which grows denser as the poles are brought closer together, but is unlike that from bodies "giving" or "receiving" electricity, that is unlike the discharge from a positive or a negative

* *Phil. Trans.* for 1770, pp. 192-210; reprinted in the *Experiments and Observations . . . relating to Natural Philosophy*, vol. ii (1781), p. 258.

† *Loc. cit.*, p. 209.

pole. It is in these experiments of Priestley, so long overlooked, that we find the first record, so far as I know, of an oscillatory discharge, that wave-motion on which wireless is based. One can imagine what a Faraday would have done if Priestley's work, published in the *Philosophical Transactions* for 1769 and 1770, had been brought to his notice.

It was in 1853 that Lord Kelvin showed theoretically that the ordinary discharge of a Leyden jar under certain conditions is oscillatory, a result confirmed experimentally by Feddersen.

The discoveries by Priestley of which I have spoken were made independently over again in 1888 by Sir Oliver Lodge in his remarkable work on what he calls the "Alternative Path" and the "Side-Flash." *

Priestley had been drawn by a systematic study of the history of electricity into electrical experiments. It was through his electrical experiments that he was attracted to chemistry. From 1770 till his death his scientific work is almost entirely chemical; and for the progress of chemistry, and some aspects of vegetable and animal physiology, it was of fundamental importance, so fundamental that much of it is found in every text-book to-day. But to appreciate it, and still more to attempt to appreciate Priestley's own ideas and struggles, we have, to use the phrase of Meyerson, to attempt to divest ourselves of that shirt of Nessus, the scientific environment and nomenclature of our own time, and to put ourselves, as far as we can, in his place.

Priestley was, unfortunately, not widely read

* See the *Electrician* for 1888, vol. xxi, pp. 234, 276, 302, and Lodge's book on *Lightning Conductors* (1892). See also Postscript, p. 34, below.

or practised in chemistry. He was, as he tells us, to the end of his life "no professed chemist." Let us for a moment examine his starting point. I have already spoken of Priestley's debt to Newton. For the details of chemical theory he is indebted to Becher and Stahl, the authors of the phlogiston theory, and to some extent to the readable and mostly reasonable Boerhaave. In experimental work he was the disciple of Boyle and of Stephen Hales, of Cavendish and, to a lesser extent, of Black.

The progress of science is not uniform, it is constantly being sidetracked or blocked by the logical development of hypotheses which have exhausted their usefulness. For half a century or more chemistry was dominated and limited by the phlogiston theory of Stahl, which was elaborated between 1700 and 1730, and of which, despite its familiarity, I ought, perhaps, to say a word or two in explanation.

From the fact that fire destroys most constructed things, Stahl regarded burning as a decomposition. All combustible things, he thinks, owe their combustibility to a fiery principle or earth as he calls it; this fiery principle, or *phlogiston*, escapes in the process of burning. The theory had the initial advantage of classifying together phenomena so different as the calcination of metals, the combustion of wood, the respiration of animals.

Where we now say that a body has been oxidised, the disciples of Stahl would have said that it had lost phlogiston. Where we say that iron and oxygen combine to form oxide of iron, the phlogistonists said that iron is decomposed to form a calx of iron plus phlogiston.

What we call an oxide of metal they called its calx. The metal iron is for them a compound body made up of calx of iron and phlogiston. All metals contain phlogiston. When you burn them phlogiston becomes free. When you dissolve them in acids, like sulphuric acid or hydrochloric acid, phlogiston escapes ; the escaping gas (hydrogen) is a body rich in phlogiston, if not phlogiston itself.

Priestley, only having a single theory to deal with, instead of two as in electricity, in the beginning accepts the phlogiston theory as a pupil, with docility.

But it was not the phlogiston theory which alone blocked the way to advance ; there were three main pitfalls of ignorance which had to be filled in :

(1) It was not known whether heat was ponderable or not. Boyle thought it was, and that it could penetrate a heated closed glass vessel and unite with the metal contained therein.

(2) It was not known whether light was ponderable or not, but chemistry was less affected by the ignorance with regard to light than by ignorance with regard to heat.

(3) Despite the investigations of Boyle and Mariotte on the weight and compressibility of the air, permanent gases or airs were regarded as of an entirely different character from solids and liquids. It was van Helmont, the Flemish chemist, in the seventeenth century, who invented the word gas and derived it, as it seems, from the Greek *chaos*.* Boerhaave, using the same word, regards the air as a "universal chaos" in which

* See van Helmont's *Works, made English*, by J. C. (1664) Ch. XI, § 28, p. 69 ; and M. Speter, *Chemiker-Zeitung*, vol. 34 (1910), p. 193.

corpuscles of almost every kind are confounded together, in which there float the attenuated particles of all bodies, something which we never know pure, which may even be without weight if it is pure, and varying in properties with the impurities contained in it, fire, water, the smells of vegetables, the scent of animals and plants, the dust of the earth, the vapour of liquids, etc.* Boerhaave regards air in itself, like fire, as an instrument influencing chemical actions rather than taking part in them. We know definitely that Priestley studied Boerhaave.† It is only with the views of Boerhaave and his school in our mind that we can understand some of the more extraordinary suggestions of Priestley and his friend Kirwan with regard to the constitution of gases.

It was Priestley's reflexion that charcoal, like metals, was a conductor, and like metals contained phlogiston, that led him to make one of his first chemical experiments, if not the first. Knowing that charcoal will not consume by heating except in the open air, and believing that charcoal like a metal consists of an earth united to phlogiston, he heats lead in a crucible, after covering it with pipeclay and sand to keep it from contact with the air, and finds that it is only slightly calcined or vitrified.

But Priestley's great service to chemistry was the discovery and investigation within a few years of nine new gases, where only three had been generally recognised by chemists before—ordinary air, carbonic acid (known as *gas sylvestre*

* Cf. *Elements of Chemistry*, by A. Boerhaave, trans. by T. Dallowe, 1735, vol. i, pp. 247–317, and especially pp. 268, 271, 273 and 282–293; also Hélène Metzger, *loc. cit.*, p. 249.

† *Memoirs* (1806), p. 183.

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or fixed air), and inflammable air (hydrogen). Giving them their modern names, Priestley's new gases were hydrochloric acid, sulphur dioxide, nitrogen,* nitric oxide, nitrous oxide, ammonia, oxygen, silicon fluoride, and sulphuretted hydrogen.

It was not by design but by chance that Priestley was led to investigate what he himself calls "the doctrine of air," about the year 1770. He was living in Leeds next door to a brewery, and examined the fixed air produced by fermentation; and then, removing to another house, had to generate it for himself. If it was only by chance that Priestley was led to study air, his great memoir of March, 1772, marks an epoch in the history of science.†

He improves Hales's methods of collecting gases in the pneumatic trough, and by using mercury instead of water, is able for the first time to collect soluble gases. He thus discovers marine acid air (hydrochloric acid gas). Following up some observations of Hales, he discovers "nitrous air" (nitric oxide), and finds that in the presence of water it removes the "good" part of the air which supports life and combustion. Previously he had estimated the goodness of air by the time that a mouse would live in it, but he now substitutes nitric oxide for mice, and so lays the foundations of exact eudiometry. He shows that in air exposed over water one-fifth disappears in processes of combustion, respiration, and putrefaction, and that living green plants restore air vitiated by these processes. He also suggests the making of what we now call soda water, by saturating water with "fixed

* Daniel Rutherford isolated nitrogen about the same time and independently.

† *Phil. Trans.* for 1772, pp. 147-264.

air " (carbonic acid) under pressure. Priestley describes also the preparation of pure nitrogen, to which he gives the name "phlogisticated air," recognising it only later as a distinct species. He notes without comment the production of two other gases, subsequently recognised as new (now called carbon monoxide and nitrous oxide), and he obtains a gas from nitre which further examined would have proved to be the then undiscovered oxygen.

In a number of passages scattered throughout his works, Priestley attributes his discoveries to the kind of chance which favours a dog hunting for game, and he has been taken at his word by indiscriminating critics.* I think that he adopted this view largely owing to pique, produced by the more systematic expositions of his great rival, of whom I shall speak presently. This memoir of 1772 at any rate shows constant thought and planning and scientific imagination. It is by no chance that after he has found that air is vitiated by life and combustion he seeks for some cause of the restoration of the air and finds it in vegetation. Certain experiments had led him to think that agitation with sea-water might have a similar effect, though later he changed his mind about it.† Schloesing, in 1880, suggested that it is sea-water which maintains the partial pressure of carbonic acid in the air constant, the tension of dissociation of the bicarbonates in sea-water being approximately equal to that partial pressure.‡

Up to this point Priestley got hold of the right

* See for instance, *Experiments and Observations on . . . Air*, vol. i (1790), preface, p. xxi.

† *Experiments and Observations on . . . Air*, vol. i (1774), p. 269.

‡ *Comptes Rendus*, vol. 90, p. 1410.

end of the stick a good many times. But he suffers in comparison with that greater contemporary, ten years younger than himself, Antoine Laurent Lavoisier, who, under the constant stimulus of Priestley's discoveries, was, in a few years, to transform chemical science. The interrelations between Priestley's work and Lavoisier's are so close and repeated that we cannot understand the position of either man without knowing something of the other.

Lavoisier had all the advantages of fortune and scientific education that had been denied to Priestley; he had received the best scientific training that Paris could give him in mathematics, physics, chemistry, geology. From the very first he worked balance in hand, and showed in 1770 that the residue found after boiling water for a very long time in glass was due to the glass, which had lost a weight equal to that of the residue. But the great stimulus to him came from Priestley's memoir of 1772. In November of that year he convinced himself that the conversion of sulphur into sulphuric acid, and of phosphorus into phosphoric acid, involves a gain and not a loss of weight, and that the increase is due to the fixation of what he calls "a prodigious quantity of air." He announced the results in a sealed packet deposited with the French Academy of Sciences, and only ventured to publish them later. But we know from his laboratory notebook of February, 1773, that he then foresaw that the study of gases would lead to a revolution in chemistry, and he set out to master the history of his subject, as Priestley had done with electricity.

In 1774 Lavoisier begins his *Opuscules physiques*

et chimiques, with a long historical analysis of work done on the *elastic emanations evolved from bodies during combustion, fermentation, and effervescence*, from the time of Paracelsus onwards ; he explains that these emanations correspond to the gas of van Helmont, the factitious airs of Boyle and of Cavendish. He studies in detail the work of Hales, and of Black, for whose genius he expresses great admiration.

You remember that Black had shown in 1755, balance in hand, that caustic alkalies like lime are converted into fixed alkalies by the absorption of what he called "fixed air" (our carbonic acid), and that this fixed air could be recovered from them by heating them and by the action of acids. It seems strange to us now that Black's work remained for so long isolated and unproductive.

Lavoisier in this preliminary essay plays the part of the cautious historian ; if he admires Black, he treats with great politeness Black's critics, Meyer and Cranz. He concludes his summary by an analysis of the work of Priestley, which he describes as the most laborious and interesting since that of Hales, and apologises for making his abstract of it almost as long as the original.* The historical summary is followed by a number of experiments on combustion, of which some are of the first importance. Lavoisier confirms Priestley's observations that in the calcination of metals in air the volume of air is reduced, but he points out that there is an increase in the weight of the metal corresponding to the decrease in the weight of the gas, and says

* The summary is supplemented by extracts or reprints of articles by Duhamel, Rouelle, Bucquet, and Baumé.

that Priestley, who regarded the diminution of the air as due to phlogistication, appears not to have suspected that, to quote Lavoisier's own words, "calcination itself is an absorption, a fixation of the elastic fluid." *

He followed this up in a memoir of which he read an extract before the French Academy of Sciences in November,† 1774, but only handed in in 1777. He tells us how he repeated the experiment of Boyle, of a century earlier (1673), on the calcination of tin in a closed vessel, and found that, contrary to the conclusion of Boyle, the closed vessel and tin together weigh exactly the same after heating as before ; and that the increase in weight of the tin was equal to the diminution in weight of the enclosed air.‡

Lavoisier himself regards his experiments as incomplete ; but they show that a given quantity of air can only calcine a given quantity of tin, and only a portion of the air can be absorbed. He suggests that the air is either a mixture or a compound.

Between the publication of the *Opuscules* and Lavoisier's memoir of November, 1774, something important had happened. Priestley had, on August 1, 1774, heated the red precipitate of mercury or calx of mercury, and had obtained, to his surprise, an air in which a candle burnt with splendour and a piece of red-hot wood

* Lavoisier, *Oeuvres*, vol. i, 621 and *passim*.

† On November 12. The extract as he read it was reprinted in Rozier's *Observations sur la Physique* for December, 1774, vol. iv, pp. 448-453 (in some copies, as Meldrum points out, pp. 446-451). The full version of 1777, which is not only extended, but altered in essential particulars, is printed in the *Oeuvres*, vol. ii, p. 105.

‡ According to B. N. Menshutkin, M. W. Lomonossow had disproved Boyle's statement in 1756, but his work remained unknown (*Ostwald's Annalen der Naturphilosophie*, vol. iv, p. 219 ; and *Journal of Chemical Education*, vol. iv (1927), p. 1079).

sparkled. This was, of course, oxygen. He tells us in Volume II of his *Experiments on Air*, published in 1775 (p. 36), that being in Paris in the October following, he had frequently mentioned his surprise at the kind of air he had got to Mr. Lavoisier, Mr. Leroy, and several other philosophers. But he could not have given them much information, for he also tells us that, being busy with other work, he continued "in ignorance of the real nature of this kind of air" to March 1 following, and that he had so little suspicion of its being wholesome that he had not even thought of applying to it the test of nitrous air.* Did Priestley put Lavoisier on the track of experimenting with the red precipitate? Very possibly. He could not have done more than that. Clearly Lavoisier did not isolate oxygen in November, 1774, or he would have said so.

The public announcements made by the two men with regard to the new gas were almost simultaneous, and the form of those announcements strikingly illustrates the difference of their outlook. I begin with Priestley.

In a letter of March 15, 1775, Priestley describes his discovery of oxygen to Sir John Pringle, President of the Royal Society,† and says: "As I think I have sufficiently proved that the fitness of air for respiration depends upon its capacity to receive the phlogiston exhaled from the lungs, this species may not improperly be called dephlogisticated air." On April 1 he writes a letter to Dr. Richard Price, F.R.S., in which he suggests that red lead, and *mercurius calcinatus per se* (mercuric oxide), etc., extract

* *Experiments . . . on Air*, vol. ii, 1775, p. 40.

† *Phil. Trans.* for 1775, p. 387.

nitrous (nitric) acid from the air, and that this acid is the most essential among the various ingredients which compose the atmosphere. He regards oxygen about this time as being composed of an earth and nitrous acid, because the red precipitate could be got from mercuric nitrate and because oxygen could be got from other earthy nitrates. We seem to have here the chaos theory of Boerhaave.*

In a letter of May 25 to Pringle he writes that "the purest air is that which contains the least phlogiston: that air is impure (by which I mean that it is unfit for respiration and for the purpose of supporting flame) in proportion as it contains more of that principle; and that there is a regular gradation from dephlogisticated air (oxygen), through common air, and phlogisticated air (our nitrogen), down to nitrous air (our nitric oxide)," the nitrous air containing the most and the dephlogisticated the least phlogiston possible, and that they have a common basis—nitrous (our nitric) acid. But he ends the letter with the characteristic and important phrase, "I lay no stress upon any opinions farther than as they may lead to the discovery of new facts." The three letters from which I have quoted were read at the Royal Society on May 25, 1775.†

It was a little earlier, on April 26, 1775, that

* See *Expts. on Air*, vol. ii (1775), pp. 55, 62. Priestley changed his mind about oxygen several times. In 1779 he thought it might sometimes contain sulphuric acid or another acid equally related to sulphuric and nitric acid (*Expts. and Obsns. relating to Nat. Philosophy*, vol. i (1779), pp. 198–9, and 260–269). In 1786 he tells us that he then thought it was nitric acid plus heat, but that Watt persuaded him to regard it as one of the constituent parts of water combined with heat (*loc. cit.*, vol. iii (1786), p. 290). But on p. 402 of the same volume he suggests that it is an element plus heat; and this view I think he retained thenceforward.

† *Phil. Trans.* for 1775, pp. 384–394.

Lavoisier read his paper in continuation of the one on tin, entitled "The nature of the principle which combines with metals during their calcination and increases their weight." It was read again on August 8, 1778.*

Lavoisier raises at once these questions—obviously with the view of Boerhaave in his mind: Are there different species of air? Are the different airs separate substances or only modifications of atmospheric air? We have just seen that for Priestley at this time what we now call nitric oxide, nitrogen and oxygen were all modifications of ordinary atmospheric air. Lavoisier suggests that what combines with a metal to form a calx is not a portion of the air but the air itself, and says he tried to obtain it from a calx which could be reduced without any addition. The only one to give satisfactory results was the red mercury precipitate. But could this be proved to be a calx? He reduces it with charcoal and finds that it yields a gas with all the properties of fixed air (carbon dioxide) like other calces. Then he heats it by itself with a burning glass and obtains a gas which is not fixed air (carbonic acid), because it does not precipitate lime water or unite with alkalies, but which supports life and in which combustion takes place with astonishing rapidity. This, the "purest portion of the air," he says, is the principle which we breathe, which unites with metals when they are calcined, which unites with charcoal to yield fixed air.

* The original text is obviously that printed in Rozier's *Observations sur la Physique* for May, 1775 (vol. v, pp. 429-434), and it is to this text that I refer here. The revised text of 1778, which differs from the original in some essential points, is reprinted in the *Oeuvres*, vol. ii, pp. 122-128. Priestley criticised Lavoisier's paper in his *Expts. and Obsns. on Air*, vol. ii (1775), p. 320.

The two memoirs mark the parting of the ways between Priestley and Lavoisier. Lavoisier's memoir, though not free from contradiction and mistake, sets him plainly on the track to effect that revolution in chemistry he had foretold. Revolution is the word which the unconvinced Priestley himself applied later to the transformation in men's minds.

The discovery of oxygen has given rise to controversy. In his memoir of 1775 Lavoisier does not refer to Priestley. Later, in a memoir of 1782, Lavoisier speaks of the gas "which Mr. Priestley discovered at very nearly the same time as I, and I believe even before me," * and in his *Traité élémentaire de chimie*, published in 1789, he says, "this air which M. Priestley, M. Scheele and I discovered almost at the same time." †

It was not till 1800, after Lavoisier's death, and when Priestley's memory was failing him, that he made Lavoisier's claims a matter of public complaint.‡

I confess that I do not think that Priestley's claims to fame rest mainly on the discovery of oxygen. He had isolated the gas in his laboratory in or before November, 1771; § he did not identify it in any way till 1775; Scheele had observed and identified it as a new gas two years previously, and his discovery must soon have become public. The importance of oxygen

* *Oeuvres*, vol. ii, p. 424.

† *Oeuvres*, vol. i, p. 38.

‡ *Doctrine of Phlogiston established*, p. 88. Dr. W. Henry reports a private letter of Priestley of Dec. 31, 1775, in which he says that Lavoisier ought to have acknowledged the account he had given him. *Report of the British Association for 1831* (2nd edition), p. 71.

§ See *Expts. and Obsns. on Air*, vol. i (1774), pp. 155-157, and *Expts. and Obsns. relating to . . . Natural Philosophy*, vol. i (1779), p. 194.

arises not from Priestley's work or Scheele's, but from Lavoisier's. It was the gas he was looking for. For myself, I find it difficult to believe that a man of Lavoisier's noble character, who so generously recognised Priestley's work in other ways, would have made claims to which he had no right. Lavoisier constantly refers to the gas as the dephlogisticated air of Mr. Priestley.

It was Priestley's fate to contribute further, though indirectly, by important observations, to the overthrow of the phlogiston doctrine. He and his friend Warltire noticed in 1777 that when hydrogen and oxygen (I use the modern names) are exploded a dew is formed, and Priestley had also noted incidentally that when a spark is fired in a mixture of air and hydrogen in the presence of water an acid is formed.

These experiments led Cavendish to show that when hydrogen and oxygen are exploded in the proportion of 2 to 1 by volume the only product is water ; and also to the experiments, which in recent years have become almost equally famous, and in which he showed that nitrogen could be converted almost but not quite entirely into nitric acid by sparking with oxygen in the presence of water. We now know that the residue consists mainly of argon.*

Cavendish interpreted his experiments according to the phlogiston theory. "Dephlogisticated air" (oxygen) "is in reality nothing but dephlogisticated water," or "in other words . . . water consists of dephlogisticated air united to phlogiston," while "inflammable air (hydrogen) is either pure phlogiston, as Dr. Priestley and Mr.

* See Cavendish's Experiments on air, *Phil. Trans.* for 1784, pp. 119-153, and for 1785, pp. 372-384—especially pp. 379 and 382.

Kirwan suppose, or else water united to phlogiston," and he prefers the latter explanation. So that water is partly composed of hydrogen, which itself contains water. Yet Cavendish was a very able man.*

Lavoisier, on the basis of Cavendish's experiments and those of himself and others, declared that water was composed of hydrogen and oxygen ; he decomposed it in the form of steam by passing it through an iron tube, and so obtaining the hydrogen while the oxygen remained behind in the form of iron oxide.

I shall not discuss here the question of priority in the discovery of the composition of water, and the relative merits of Cavendish, Watt, and Lavoisier, on which books have been written. They do not affect the position of Priestley.

It has been stated that Priestley never grasped the new theory, that he never wavered in his adherence to the old one, that he did not perform gravimetric experiments. But his memoirs in the *Philosophical Transactions* for 1783 and 1785 destroy these legends. Priestley wavered in 1783. He tells us that he found Lavoisier's theory so specious that he was tempted to adopt it.† But he makes experiments which seemed inconsistent with it—especially experiments which he thinks show that inflammable air (hydrogen) is absorbed by calces (oxides). Lavoisier retorts, rightly enough, that Priestley must have overlooked the water formed.‡

He wavered again in 1785, on the strength of

* Priestley adopted Cavendish's view for a time, but he obviously felt uncomfortable about its logic in 1786 ; see his interesting *Observations relating to Theory*, in the *Expts. and Obsns. relating to . . . Natural Philosophy*, vol. iii, p. 406.

† *Phil. Trans.* for 1783, p. 400.

‡ *Oeuvres*, vol. ii, p. 345..

beautiful experiments of his own, obviously made to meet Lavoisier's criticism, though he does not refer to it. In fact, he confirms Lavoisier's prediction. First of all he calcines iron in a vessel containing oxygen and standing over mercury (I use the modern terms to save time), and shows that the weight gained by the iron is equal to the weight lost by the oxygen. He then reduces the iron oxide to iron by means of hydrogen, and shows that the two processes can be repeated indefinitely. He shows again that the volumes of hydrogen and of oxygen involved are as 2 to 1, just those which combine without residue by means of the electric spark.* His figures show that the ratio of the loss of weight of the iron oxide to the weight of water formed is as 15 to 17. The figures of to-day would be 16 to 18.† The inaccuracy is trifling for the period.

He interrupts the account of his experiments to say that it appears to him very evident that water, with or without fixed air (this was due to carbon as an impurity in the iron), was the product of the inflammable air (hydrogen) and the pure air (oxygen) let loose from the iron in this mode of operation, but he adds also, "I was taught by Mr. Watt to correct this hypothesis and to account for this result in a different manner"‡—by James Watt to whom the discovery of the composition of water has been often attributed!

I cannot follow Priestley through all the experiments and ingenious complexities of this remarkable memoir, in which he was largely misled by confusing, not alone, the "heavy inflammable air," which consisted of carbon monoxide, or water

* *Loc. cit.*, p. 285.

† *Loc. cit.*, p. 286.

‡ *Loc. cit.*, p. 286.

gas, with the "light inflammable air," or hydrogen. Priestley henceforward displays what seems to us almost a perverse ingenuity in adapting the phlogiston theory to fit every new fact.

Meanwhile, Lavoisier gets over nearly all the difficulties of which I spoke earlier. By his experiments and an impeccable logic he destroys the phlogiston theory; he shows that gases have no special chemical qualities distinguishing them from bodies in the solid and liquid state; that in a chemical change no weight is lost; and he builds up a new gravimetric chemistry with a new nomenclature to fit it. He adopts a quantitative theory of heat, which, though not our own, treats it as having no weight.*

Black was converted; but, like Scheele in Sweden, the great British trio, Cavendish, Watt, Priestley, remained obdurate—Priestley, obdurate but indefatigable. He had a constitutional difficulty in accepting the work of others, yet was never content with his own. Quite late in life he discovered some of the most serious sources of his early errors, due to the exchange of gases between a vessel inverted over water and the outside. Some of his errors are due to diffusion through heated clay-pipes, others, perhaps, to diffusion through heated cast-iron, only discovered in the 19th century by Troost and Deville. Yet he was alive to the problem of diffusion and showed that two gases of different specific gravities (inert to one another) when once mixed would not separate.†

* See *Oeuvres*, vol. ii, p. 337. The nomenclature was devised by Lavoisier with de Morveau, Berthollet and Fourcroy.

† He says, however, "I do not say but that if two kinds of air, of very different specific gravities, were put into the same vessel, with very great care, without the least agitation that might [not] mix or blend them together, they might continue separate, as with the same care wine and water may be made to do: but that when once they have

Perhaps the most serious of his stumbling-blocks was the confusion of the two inflammable airs, a confusion shared by some of the French chemists and only cleared up by Cruickshank's discovery of carbon monoxide in 1801. But Priestley, though he had admitted by then that carbonic acid contained oxygen, could not accept a combustible oxide. *

In a pathetic pamphlet published in 1796,† in which he confesses that he is almost alone in his views, and in his last work, *The Doctrine of Phlogiston Established and that of the Composition of Water Refuted*, of which the first edition appeared in 1800, and the second in 1803, just before his death, Priestley repeats his old arguments, but he solves the puzzle of his perversity. In forming a theory, he says, we must content ourselves with the fewest difficulties. He obviously persuades himself that he is true to the old view expressed in the *History of Electricity*, that hypotheses do not really matter except as stepping-stones. He is still willing to be converted to the new theory if he can be shown that it offers fewer

been mixed, they will continue to be so, like wine and water, after having been shaken together." (*Expts. and Obsns. on . . . Air*, vol. iii (1777), p. 304.)

* The confusion is well illustrated in the *Expts. and Obsns. relating to Nat. Philosophy*, vol. iii (1786), p. 442 *et seq.* Priestley in later life wrote paper after paper on the crucial work of Lavoisier on the decomposition of steam by iron, and pointed out that the gas produced often yielded fixed air (carbon dioxide), which no one could explain. Yet Priestley himself was on the track of the true explanation. In the edition of his *Expts. and Obsns. on Air* of 1790, vol. i, p. 308, he had devoted a chapter to the "analysis of different kinds of inflammable air;" and he had shown in 1791 the difference between cast iron and malleable iron: the gas produced by dissolving the first in acid yielded fixed air on combustion, the other none (*Phil. Trans.* for 1791, pp. 221, 222). Another of his difficulties was the presence of nitric acid after combustion, due to nitrogen as an impurity in his gases. (See *e.g. Phil. Trans.* for 1791, p. 213.)

† *Considerations on the Doctrine of Phlogiston, etc.*

difficulties than the old one. Philosophically he feels himself to be sound.

I have had to pass over many details both of Priestley's achievements and of his mistakes, but three further investigations are of such general importance that they must be mentioned, one of physical and two of biological importance.

He recorded in 1781 a series of experiments on hydrogen, oxygen, carbonic acid, and common air, showing that the intensity of sound produced by striking a bell in a gas depends solely on its density and not on its chemical composition.*

In 1776 he published a memoir on respiration and on the use of the blood.† He regards the action of the blood in the lungs as phlogisticating the air, while ordinary air and dephlogisticated air turn the dark venous blood into the florid arterial. Nitric oxide, hydrogen, and carbonic acid (I use modern names) all turn blood dark, but oxygen turns the dark blood red. By careful experiment he shows that the oxygen can penetrate a layer of serum to the dark blood below and act on the red corpuscles. Once more Priestley serves as the acknowledged starting point for Lavoisier. Lavoisier's great researches published alone and with others on animal respiration and animal heat originated in Priestley's memoir. ‡

In 1778 Priestley took up again the delicate work, "on the melioration of air by the growth of plants," begun in 1771.§ He showed that oxygen was sometimes contained in the bladders of fresh

* *Expts. and Obsns. relating to Natural Philosophy*, vol. ii (1781), p. 295.

† *Phil. Trans.*, 1776, pp. 226-248, reprinted in the *Expts. and Obsns. on Air*, vol. iii (1777), pp. 55-84.

‡ See Lavoisier, *Oeuvres*, vol. ii, pp. 174, 283 and *passim*.

§ *Expts. and Obsns. relating to Natural Philosophy*, vol. i (1779), pp. 296-360, and vol. ii (1781), pp. 1-63.

seaweed and was given off by conferva and other green plants under the action of light. He was partly anticipated in publication by Ingenhousz. In one of his very last papers written in November, 1803, he defends against Erasmus Darwin, by means of fresh and interesting experiments, his old view that the green conferva arise from seed carried through the air and that there is no spontaneous generation.* He has once more got hold of the right end of the stick. His intelligence remains intact, bright, and lively to the end.

Perhaps, speaking within these walls, it is not out of place to mention that Priestley wrote from America to welcome the young Davy to the world of science. And it is from some notes for a lecture delivered in this Institution on January 27, 1810, by John Dalton (published by Roscoe and Harden †) that we know that Dalton's Atomic Theory was derived from the consideration of mixed gases, in connexion with a proposition from Newton's *Principia* and the work of Priestley on diffusion, to which I referred above. Once more, then, Priestley, though in an odd way, serves as the originator of a chemical revolution.

I made it my business, a good many years ago, to read through all Priestley's scientific books and papers. It is easy to understand why both his work and his personality have been underestimated in the past. His electrical work and his clear views on scientific theory have been

* I ought to add that he argues in it incidentally against Erasmus Darwin's theory of evolution, for which he sees no evidence. The paper was read to the American Philosophical Society on Nov. 18, 1803, and is published in vol. vi (p. 119) of their Transactions, issued in 1809.

† See *A New View of Dalton's Atomic Theory*, by H. E. Roscoe and Arthur Harden (1896), p. 13 *et seq.* See also *Manchester Memoirs*, New Series, vol. i (1805), pp. 259 and 268.

eclipsed by his chemical work ; and in judging of his chemical work it is no simple task to divest his language of that enveloping veil, the phlogiston theory, or to divest oneself of one's own modern views, and to see him, as it were, face to face. But the record of his achievement which I have put before you shows, I think, that he deserves a greater place in the history of science than he has been accorded hitherto. There have been greater men ; but, even so, there have been few Priestleys, few men with a more amazing instinct of the right subject to investigate, few men whose work has borne a richer harvest.

Postscript.—In this estimate of Priestley I have made no attempt to discuss exhaustively the controversial subject of the “discovery of oxygen,” or to estimate the importance of Priestley's predecessors, Jean Rey and John Mayow, or of his contemporaries, Bayen and Beccaria. While I disagree in some ways with Dr. A. N. Meldrum in his judgment of Lavoisier, the best recent work on the subject is, in my opinion, his *Eighteenth Century Revolution in Science—the First Phase* (Longmans, 1929). A great deal of information is given in a memoir by S. M. Jørgensen (who agrees, like Rodwell,* with my general view of Lavoisier), *Die Entdeckung des Sauerstoffs*, in Ahrens and Herz's *Sammlung chemischer . . . Vorträge*, vol. xiv, 1909, and in one by M. Speter, *Lavoisier und seine Vorläufer* in vol. xv (1910), of the same journal. I have no space here to deal in detail with these memoirs. There are some interesting details about Priestley's scientific apparatus in a series of papers in the *Journal of Chemical Education* for February, 1927. I have myself given an estimate of

* *Nature*, vol. 27 (1882), p. 8.

Mayow in the Dictionary of National Biography.* A further study of Mayow, by Professor T. S. Patterson, is in course of publication in *Isis*.

Since the delivery of the foregoing discourse, a suggestion by Professor William Cramp, of Birmingham, has led me to find that Priestley was anticipated in the discovery of the "lateral explosion" (but not of its oscillatory character) by the interesting Benjamin Wilson, F.R.S., electrician and portrait painter [1721–1788], like Priestley, a Leeds man. His experiment, which Priestley must have overlooked, is described in his *Treatise on Electricity* (1750), p. 89.† The experiment is a very simple one, not comparable with Priestley's elaborate investigation, and the sparks he obtained did not exceed $\frac{1}{20}$ -inch in length. Wilson claimed priority for himself by publishing a letter on the subject from E. H. Delaval, F.R.S., in his *Observations upon Lightning* (1773), p. 29. Viscount Mahon (afterwards 3rd Earl Stanhope) refers with praise to Priestley's work on the oscillatory discharge in his *Principles of Electricity* (1779), p. 147.

P. T. Riess, in his paper "Ueber die Seitenentladung der elektrischen Batterie" in the *Abhandlungen der k. Akad. der Wissenschaften, Berlin*, for 1849, pp. 1–34, refers first to Priestley, and gives a list of writers who investigated the lateral explosion subsequently to him, including William Henly, Sturgeon, Snow Harris, Wheat-

* I may note that the editors of that work, on evidence which seems to me entirely insufficient, altered the date of Mayow's birth, as given by me in the first impression of 1894, from 1643 to 1640. The only indication that changes have been made in subsequent impressions is a printed slip, and I have only just discovered the change referred to.

† Wilson gives an illustration of the experiment, but with an obviously wrong reference (plate 10 instead of plate 9).

stone and Faraday. The references to the last four obviously relate to a controversy in 1839-40 on the value of Snow Harris's lightning conductors for the navy, a controversy in which the lateral explosion figures largely. The only possible allusion I can find to the oscillatory nature of the discharge is in a statement of Sturgeon, who in his *Annals of Electricity*, vol. iv, pp. 174 and 501, speaks of *waves* of electricity being produced by the lateral discharge, but does not define the waves further.* Sturgeon's *Annals*, vols. iv and v, contain a long correspondence between Harris and Sturgeon, and vol. v gives an abstract of the Admiralty Report by the Commission appointed to inquire into Harris's method of protecting ships from lightning (printed *in extenso* as a Parliamentary Paper, February 11, 1840). Faraday's evidence in the Report contains the only reference I can find in Faraday's work to the lateral explosion. The passage in question (*loc. cit.*, p. 34) reads as follows: "With regard to the 'lateral explosion' he [Faraday] was not aware of any phenomenon so called, which was not a diversion or a division of the primary current. The returning stroke, which has by some been confounded with the lateral discharge, is a distinct phenomenon, and as it occurs at a great distance from the descending discharge cannot affect the same ship." Wheatstone, who, like Faraday, gave evidence in support of Harris, says, incidentally, that the lateral discharge was first observed by Henly, but gives no reference (*loc. cit.*, p. 96). Henly's first paper was

* In a passage in a paper in his *Scientific Researches* (1850), p. 361, originally read at the British Association in 1839, Sturgeon also mentions waves of electricity produced by the lateral explosion, but gives no explanation of his meaning.

published in 1772; and the only paper by him on the lateral explosion that I can find is one in the *Phil. Trans.* for 1774, pp. 401-403, so that Wheatstone's statement seems to be without foundation.

Helmholtz seems to have been the first, in more modern times, to suggest the oscillatory nature of the discharge of a Leyden jar, in his *Erhaltung der Kraft* in 1847 (Ostwald's reprint, p. 33). See Thomson's *Mathematical and Physical Papers*, vol. i (1882), pp. 534-553.

I wish to add a reference to Priestley's interesting experiments on diffusion through unglazed retorts in his memoir on the *Seeming Conversion of Water into Air* (*Phil. Trans.* for 1783, pp. 414-434, and the *Experiments and Observations . . . on Air* of 1790, vol. ii. pp. 407-435). Dalton refers specially to this work.

Priestley's views on the "physical point" theory of Boscovich are explained in his *History of . . . Light*, vol. i. (1772), p. 390.

Priestley's letter of protest to Sir Joseph Banks with regard to the rejection of Cooper by the Royal Society (dated April 25, 1790), is reprinted in an interesting memoir, *Contributions to the History of Science*, by Kurt Loewenfeld, in the *Manchester Memoirs*, vol. 57 (1912-13).

Miss Anne Holt's recent *Life of Joseph Priestley* (Oxford University Press, 1931) does not claim to add anything to the knowledge of his scientific work.

I am sorry not to have been able to touch on Priestley's work in education. But I understand that Sir Michael Sadler will discuss this subject in an introduction to a reprint of Priestley's *Remarks on a Code of Education*, to be published by the Cambridge University Press.—P. J. H.